We thank the two anonymous reviewers for their valuable and critical reading of the manuscript, which were useful in improving the scientific quality and clarity of the work. A point-wise response is given below, with the original reviewer comments in italics, and our answers in blue and labelled as "Response". Please note that all line numbers refer to the tracked changes version of the manuscript.

Kind regards,
Kirsty Edgar and co-authors

RESPONSE TO REVIEWER 1

General comments

The article presents a well-structured summary of our current knowledge of planktonic foraminiferal ecology and clearly points out current knowledge gaps. Perspectives for future research are highlighted in the abstract and the conclusions, but they remain rather vague in the article itself, so that both the abstract and the conclusions seem to be disconnected from the main body of the article. For example, eDNA and eRNA approaches are mentioned in the abstract and/or the conclusions, but not in the main article.

Similarly, it is repeatedly stated that observational and experimental data as well as modelling approaches are needed to improve our understanding of foraminiferal ecology, but the suggestions for future studies remain rather vague. What exactly should be done in future experimental/observational studies? How exactly can trait-based models help to understand foraminiferal ecology and fill current knowledge gaps? In my opinion, this aspect needs more focus and concrete examples if it is to be emphasized in the abstract and in the conclusions.

Response: Thanks for highlighting this discrepancy. We have made a number of amendments throughout the manuscript (e.g., L464-468, 484-484) including creating a new section in place of the previous brief conclusions called "Suggestions for future research" (L584-673). This section provides more details on key aspects of each of the major traits for future experimental, observational and modelling research.

In addition, the article lacks a robust discussion of what challenges future studies may face when following the author's suggestions. Since this is a review and syntheses article, I do not expect an extensive discussion. However, I think that the reader should at least be made aware of the major challenges associated with the suggested approaches. This is already partly done for observational and experimental studies, but modelling approaches are, in my opinion, presented much too idealized. For example, it is stated in the conclusions that models "can create theoretical frameworks akin to laboratory experiments". I think it is essential to mention that models are based on assumptions and simplifications, i.e., they will never be an exact replica of nature. The only model limitation discussed in the article is the lack of input data for foraminifera. But what does this imply for the reliability of model simulations? Under what conditions and to what extent can models be compared to real-life experiments and/or observations? Such thoughts should be included in the argumentation.

Response: Agreed. In response to this comment, along with similar comments raised by Reviewer 2, we have added a new section to the manuscript discussing model limitations and challenges under a section called "*Suggestions for future research*" (L651-663) in addition to additional comments within the main text addressing minor comments that are detailed elsewhere in the reviews, e.g., L70-72 re. Gibbs et al. (2020) study and in the introduction (L84-89).

Finally, the language lacks clarity. Long sentences with punctuation problems and sometimes minor grammatical problems make it difficult to follow the argumentation of the article. However, this problem should be straightforward and easy to fix.

Response: Thank you for pointing this out. We have carefully reviewed the article to improve clarity and ensure that the argumentation is clear and easy to follow.

Specific comments

It may be helpful to specify some technical terms as not all readers may be experts in the field: L. 43: pseudopodial network; l. 45: pseudopodia; L. 95: cryptic species; Table 2: rhizopodial (rhizopodia is explained in l. 248; I would add the same short explanation to the table caption); L. 146: shell flux; L. 432: blue water

Response: Thanks for flagging this. We have now described all of these terms for the reader or use alternative phrases where there is an appropriate, clearer alternative.

L. 34: Do you mean fossil records among all phytoplankton groups"?

Response: Apologies for the lack of clarity. We intended to say that planktic foraminifera have one of the most comprehensive fossil records among ANY plankton group. We have amended the text to make this clearer (L36).

L. 54-55: There is no clear link between these two paragraphs, which disrupts the flow of the text. Maybe you could add a connecting sentence to the paragraph beginning in l. 55.

Response: Thanks for highlighting this disconnect. We have tweaked the text to make the connection clearer. For example, *"Developing a mechanistic understanding of the controls on planktic foraminifera diversity and distribution is essential to generating accurate predictions of how changing environmental conditions will impact planktic foraminifera communities and, ultimately, biogeochemical cycles (Dutkiewicz et al., 2020).*

An improved mechanistic understanding can contribute to trait-based approaches, which describe how species interact with each other and their environment based on measurable organismal characteristics or traits (e.g. size, resource acquisition mode and defence; Dutkiewicz et al., 2020)."

L. 70-72: I think it is important to state here how successful these trait-based models actually were in reproducing the sedimentary record, so that the reader can get an understanding of the current state of research. In the Gibbs et al. (2020) model, for example, the simulated evolutionary response progressed orders of magnitude too fast.

Response: Yes, we should have been clearer in acknowledging that models are not perfect solutions but rather valuable complementary tools. We have now added an explicit reference to the challenges in the example highlighted, along with a broader discussion of the limitations and caveats associated with trait-based modelling. In the introduction, L84-90, "*While trait-based models have significant potential, they have inherent limitations. Like any models, they are a highly simplified version of natural systems. They may simplify complex ecological interactions and environmental influences, and the expression and importance of traits may vary by environment impacting generalisation. Additionally, trade-offs between traits are not always well understood or quantified, as for example, the absence or presence of symbionts in foraminifera. The eco-evolutionary model used by Gibbs et al. (2020), for instance, resulted in modelled trait evolution at rates orders of magnitude faster than* *observed in the fossil dataset, highlighting the need for caution when interpreting these model results."*

We have also expanded the limitations where needed in the main text (e.g., L457-470) and in a new section on "Suggestions for Future Work" at the end of the manuscript (L651-673). See response to comments elsewhere for more.

L. 77: Since models are based on assumptions and simplifications, their results have to be interpreted with care. Therefore, I would not use the term 'vastly' here.

Response: Thank you for your suggestion. We have removed the word "vastly" here.

L. 78: The current formulation is quite misleading; do you mean the first modeling study on foraminifera?

Response: Apologies for the lack of clarity. While earlier models on planktic foraminifera exist (e.g., Žarić *et al*.,2006; Fraile *et al*., 2008, 2009; Lombard *et al*., 2011), these models are species-specific, characterising a limited number of taxa for which empirical data is available, and thus, cover a relatively narrow range of environmental conditions and groups. The Grigoratou *et al*. (2019) study referred to in the text represents the first non-species-specific trait-based model. We have now clarified this in L94-98.

L. 97-99: This sentence seems to contradict itself. Do you mean "their presence in shallow marine seas"? If not, please elaborate on this further.

Response: We have now clarified this sentence. Planktic foraminifera inhabit the pelagic zone (open ocean), which excludes shallow marine seas or coastal shelves.

Table 2: Why is an increased area to volume ratio beneficial? Is it due to the increased capture area? If yes, you should make clear that this is meant here. If not, you should explain this somewhere in the text.

Response: An increased area-to-volume ratio offered by spines probably provides multiple benefits, including increasing the prey capture area (as suggested), an important advantage in the low prey environments these taxa typically inhabit. In addition, increasing drag helps taxa to maintain buoyancy. These benefits are currently detailed in Section 2.1.2, but we have also amended the table text to explicitly state "*increased prey capture area*", as positive buoyancy is already highlighted.

L. 138-139: This sentence seems to contradict itself. What do you mean by individuals reaching larger sizes in the tropics but smaller sizes in equatorial regions?

Response: Thanks for pointing this out. The term "equatorial regions" should indeed read "equatorial upwelling regions". We have amended the text accordingly.

L. 145: Do you mean "greater reproductive success"?

Response: Yes, we have made this clearer in the amended text.

L. 172-176: Why is an increased sinking rate beneficial? This seems to contradict Table 2, where negative buoyancy is listed as a cost, while positive buoyancy is listed as a benefit.

Response: Thank you for spotting this discrepancy. The text should read that spines lead to a *decrease* (not increase) in sinking rate, helping further to counterbalance the negative buoyancy created by the shell in these taxa. We have now corrected the text accordingly.

L. 205: “also” instead of “though”?

Response: Thanks for flagging this. We have amended the text to read “*However, many studies identify*” to better reflect that while some studies indicate a group-level response, others find a species-level response.

L. 212: What exactly do you mean by “resolving the aforementioned biological factors”? Could you elaborate on this further?

Response: This sentence refers to several key aspects, including resolving cryptic species, understanding regional plasticity, identifying what controls gametogenic calcite thickness, and determining the biomineralisation pathway. We have clarified this now in L267.

L. 217-220: This sentence is very long and difficult to read, maybe split it into two sentences?

Response: Thanks for flagging this. We have split this sentence in two to improve readability and review the rest of the manuscript to revise any other long sentences to increase overall clarity.

L. 219: Do you mean reduced calcification?

Response: Yes, we have amended the text to make this clear.

L. 261: Do you mean that foraminifera can capture prey items 2-3 times bigger than themselves while most zooplankton cannot? If yes, I would write “contrary to most zooplankton”

Response: Yes, this appears to be the case. The general predator-to-prey size ratio is about 10:1, although this varies across groups, e.g., ~ 1:1 for dinoflagellates, ~8:1 for ciliates or ~18:1 for copepods (Hansen *et al.*, 1994). However, foraminifera, particularly spinose taxa, stand out as one group capable of capturing prey items much larger than their shell size. We have now made the suggested point.

L. 275-276: This sentence is difficult to understand. Maybe replace with something like “Under laboratory conditions, non-spinose species exhibit cannibalism, but whether they cannibalise in the natural habitat is unknown and considered unlikely due to very low foraminiferal abundance.”

Response: Okay, we have rewritten this sentence as suggested.

L. 278-279: This sentence is difficult to understand. Do you mean “The temporal and spatial distribution of prey is a major cause of the regional distribution of foraminiferal species, which affects growth and fecundity via temperature.”?

Response: Apologies for not being clearer. Sea surface temperature is often invoked as a dominant driver of planktonic foraminiferal diversity (and hence assemblage composition), abundance and body size (e.g., Morey et al., 2005; Rutherford et al., 1999 and Schmidt et al., 2004). However, food availability is also very important and may modify these temperature-based relationships. We have reworded this sentence as suggested while clarifying that food availability acts “*in addition to temperature*” as a key influence.

L. 316-325: This paragraph seems to contradict the previous one in which it is stated that planktonic foraminifera do not undergo diapause. Could you elaborate more on the dormancy of Neogloboquadrina pachyderma and how it differs from those of benthic foraminifera and dinoflagellates mentioned in the previous paragraph?

Response: Thanks for flagging this inconsistency. We have amended this section to clarify that *N. pachyderma* is an exception among planktic foraminifera, with evidence suggesting it can survive inhospitable conditions via dormancy. We have removed reference to how this form of dormancy differs from the diapause observed in benthic foraminifera and dinoflagellates to avoid confusion.

L. 348-358: The different variants of symbiosis described here are unknown to the reader when they read Table 1. Maybe it would be helpful to include a short definition of these terms in the table caption.

Response: Good suggestion. We have added this information to Table 1.

L. 381-382: What impact may it have on modeled foraminiferal ecology that we cannot simulate facultatively symbiont-bearing species?

Response: This is a good question and is addressed in the revised text (L457-467). We currently lack a mechanistic underpinning of facultative symbiosis validated by laboratory culture and observations to resolve the relative benefits of facultative symbiosis for the host, and the drivers of symbiosis in so-called facultative taxa. Hence, it is very difficult to model accurately. This is exemplified by a recent study developing the ForamECOGEniE model to simulate different symbiosis types (Ying *et al*., 2023), which was able to broadly replicate geographic and abundance patterns of spinose symbiont-bearing taxa and non-spinose, asymbiotic taxa but underestimated the abundance of the non-spinose symbiont-facultative group, particularly in the eastern equatorial Pacific. Intriguingly, if the abundance of the obligate and facultative symbiotic taxa are combined in the model, there is a good fit between the model and observations (see Figure 4 in Ying *et al.,* 2023). Hence, this may suggest that the groups exploit the same benefits from symbiosis.

L. 432-434: How does this sentence connect to the previous one?

Response: Thanks for pointing this out. We have amended this section to clarify that the sentence refers to a potential disadvantage of broadcast spawning and sexual reproduction - that any mismatches in the timing or location of reproduction can reduce the chances of successful fertilisation.

L. 451-453: I don't understand the part starting with "and ability". Maybe it would help to rewrite this part and put it into a new sentence.

Response: We have added a new sentence to this part to clarify that the ability to reproduce both sexually and asexually is an advantage for the group. Specifically, we have clarified that asexual reproduction is suggested to facilitate rapid population growth during the polar summer (e.g., Meilland et al., 2023). We have reworked this section to present the advantages and disadvantages of both reproductive modes more clearly (L492-527).

L. 457-459: You mention repeatedly that most traits are poorly constrained. What I am missing is a discussion of the actual potential and limitation of trait-based modeling in this regard. Which traits can be modeled, under which assumptions/simplifications can they be modeled, and which impact may that have on the informative value of the model results? I suggest discussion this briefly for each trait and giving a short summary in the conclusions.

Response: This is a good point. We have expanded on this in the revision specifically in a new section on "Suggestions for Further Research" (L585-673) and within individual sections where relevant, e.g., L580-583.

L. 478-483: I think that models are presented too idealized in this manuscript. It should at least be mentioned that models are based on assumptions and simplifications, which means that they are not equivalent to observations of real organisms.

Response: This is a good point. We have added sections in the introduction and within the manuscript to address model limitations. Please see response to comments earlier here and to Reviewer 2 for more.

Technical Corrections from L30-471

Response: We have made all of the suggested technical corrections in the final manuscript.

RESPONSE TO REVIEWER 2

Overall comments:

I think that, overall, the manuscript is well structured and does a good job of gathering all the major ecological aspects of planktic foraminifera, but also, the current knowledge gaps. I consider it is a valuable contribution in the field of planktic foraminifera ecology/modelling as it summarizes the areas that need further research pretty well. Also, it highlights the trait-based approach as a useful tool to make reliable predictions for the impact of the environmental change on these organisms.

Concerning the language, it is well-written, however, some sentences are too long and complicated, making it hard to understand the message behind them. Also, I've noticed some technical writing mistakes such as double spaces, lacking commas and dots etc…

Response: Thank you for pointing this out. We have carefully reviewed the manuscript to simplify overly long or complex sentences and correct any technical writing mistakes, as suggested.

I have mainly 3 concerns in regard to what I think the manuscript misses that should be an easy fix. I hope the following suggestions will help the authors to improve the manuscript.

- eDNA and eRNA analyses are mentioned in the: abstract, introduction and conclusion, however, there are nowhere to be found in the manuscript. I think these approaches should be given the same treatment as they are nowadays one the main ways we have to decipher some ecological aspects of the planktic foraminifera.

Response: We agree with this point and have expanded this part in a revised and renamed section of the manuscript "*Suggestions for future research*" explaining what the technique is and its potential to contribute to the study of planktic foraminiferal traits (L617-635). For now, most studies of eDNA on planktic foraminifera are largely confined to testing the approach for detecting diversity in ecosystems (e.g., *Barrenechea Angeles et al., 2020)* however, we highlight the potential insights possible using examples from benthic foraminifera and other plankton groups here.

"Environmental DNA (eDNA) metabarcoding, which analyses the genetic material present in the environment, such as sediment, or water, is a powerful new tool for identifying and

monitoring biodiversity, biogeography and reconstructing ecosystems and ecologies (Ruppert et al., 2019). This technique can also provide insights into community composition over timescales spanning several hundreds of thousands of years or longer, improving our understanding of the relationships between biodiversity, environment and climate (e.g., Ambrecht et al., 2019). However, the bulk of eDNA foraminiferal studies to date have focussed on benthic foraminifera. For instance, this technique has significantly increased the diversity of organic walled and "naked" foraminifera that are rarely observed, have few morphological characters for traditional species delimitation, and don't preserve well in the fossil record (Pawlowski et al., 2014). But eDNA holds great promise for investigating marine plankton, as it has the potential to overcome many of the data limitations that we currently face in this group with typically low standing stocks (de Vargas et al., 2015). It is a potentially more effective means of detecting species presence in an environment than observations alone (Malviya et al., 2016; Ser-Giacomi et al., 2018; Barrenechea Angeles et al., 2020) but can also contribute much more broadly. For instance, providing insights into plankton population size (Andres et al., 2023), response to environmental change (Cao et al., 2022) or predator-prey dynamics (Ruppert et al., 2019). A combination of molecular and microscopic approaches can also yield new insights, e.g., in benthic foraminifera it allowed rapid determination of multiple different feeding strategies driving diversity and abundance in several foraminiferal taxa (Schweizer et al., 2022), a question which is typically restricted to analysis of feeding vacuoles and laboratory experiments. However, further method development is still required as some groups are not as well represented by eDNA technique as others, e.g., specimens may visually be present in sediments not found in the eDNA analysis, likely because of limitations of the primer to detect certain groups (Barrenechea Angeles et al., 2020, Hoshino and Inagaki, 2024)".

- The trait-based approach is pretty well detailed, and this manuscript presents it as a powerful tool for future predictions. Nevertheless, I feel that only the positive aspects of it are presented, not the limitations. In a synthesis paper I do not necessarily expect a discussion, but I feel that the limits of this approach, other than just the lack of quality input data, should at least be described/acknowledged. Also, it remains a model, which a simplification by its own nature. Therefore, the differences between models and in-situ observations should be better described.

Response: Thanks for pointing this out. We agree that acknowledging the limitations of the trait-based approach is critical. In the revised manuscript, we have incorporated discussion of the limitations of models in the Introduction (L84-89), as part of a new "*Suggestions for further research*" section (L651-663) and where appropriate in the text, e.g., L457-460 re. modelling symbiosis. For instance, in the Introduction to also address similar comments by Reviewer 1, text reads

"While trait-based models have significant potential, they have inherent limitations. Like any models, they are a highly simplified version of natural systems. They may simplify complex ecological interactions and environmental influences, and the expression and importance of traits may vary by environment impacting generalisation. Additionally, trade-offs between traits are not always well understood or quantified, as for example, the absence or presence of symbionts in foraminifera. The eco-evolutionary model used by Gibbs et al. (2020), for instance, resulted in modelled trait evolution at rates orders of magnitude faster than observed in the fossil dataset, highlighting the need for caution when interpreting these model results. Despite these caveats, trait-based approaches present an exciting opportunity to leverage the exceptional fossil record of foraminifera to test our understanding of the modern range of environmental conditions and beyond, assess the universal applicability of traits and trade-offs through time and ultimately improve our understanding of evolutionary processes."

An example of addressing limitations of existing model-observation in Section 2.2.3 on Photosymbionts is here

"A recent study developing the ForamECOGEniE model to incorporate different symbiosis types (Ying et al., 2023) replicated patterns of asymbiotic and obligate symbiotic global distributions but underestimated the abundance of the non-spinose symbiont-facultative group particularly in the eastern equatorial Pacific. The challenge of modelling this group is the lack of information on what drives the symbiont-facultative group to acquire to lose symbionts. Without a clear ecological/physiological understanding, trade-offs cannot be incorporated in the model. Intriguingly, if the abundance of the obligate and facultative symbiotic taxa are combined there is a good fit between the model and observations (Ying et al., 2023). This may suggest that the groups overall exploit the same benefits from symbiosis. However, determining the mechanistic underpinning is critical to understand the data, particularly the environmental/biological conditions under which symbiosis is active or not, and the benefits of the relationship to the host. Thus, culture experiments and further observation of this phenomenon are still required."

- Finally, it is stated numerous times that better datasets and experimental data is needed to improve our current knowledge of planktic foraminifera. However, the ideas and suggestions for improving them remain rather vague and, in that regard, how can the trait- based approach contribute to fill the missing data? Even after reading the manuscript a few times, one would struggle to come up with a strategy to overcome those gaps.

Response: Apologies. We have attempted to expand on what types of new data are required to overcome the knowledge gaps and improve trait-based models in a new section called "*Suggestions for future research*" (L584-673). For example, for what is needed to move models forward we will detail, *"Therefore, key parameters needed to improve foraminiferal model performance include quantitative data of fundamental parameters such as: growth rate, respiration rate, half-saturation constant, and grazing preferences."* and for how models can directly contribute to constraining traits *"For modern species with high mortality rates in laboratory culture or difficulties maintaining multigenerational experiments (and high time and financial costs), trait-based models provide realistic setups to estimate potential physiological ecology in the absence of physical organisms (Grigoratou et al., 2019)."*

Considering the described comments, I think that this is a valuable and interesting contribution that should be considered for publication.

Response: Thank you!

Specific comments:

Lines 138-139. This sentence is odd. Larger sizes in the tropical regions and smaller in the equatorial region?

Response: Good spot. This should have read "equatorial upwelling" and was amended.

Lines 172-174. This seems contradictory. Here, the effect of spines increasing the sinking rate is described as a benefit, however, in Table 2, negative buoyancy is described as a cost.

Response: The text should read that spines lead to a *decrease* (not increase) in sinking rate hence, further helping to counterbalance the negative buoyancy created by the shell in these taxa. Hence, it is considered a benefit in this regard.

Lines 179-181. I think that this sentence should be re-written as it is correct, but not precise. Non-spinose species are omnivorous, they have the capacity of eating small prey such as phytoplankton, but they prefer an herbivorous diet (Hemleben et al., 1985). Also, the diet is known to change across the development of a given species. For example, O. universa shifts from herbivorous during its early and juvenile phase, to a carnivorous diet in the spherical adult stage (Schiebel and Hemleben, 2017).

Response: We have summarised the dominant feeding preferences of the adult forms here, as we provide more details later in Section 2.2.1. However, we will include a brief clarification here, while aiming to avoid excessive repetition.

Lines 203-215. I feel that this paragraph misses the chance of describing 2 mechanisms that affect the calcification in an important manner: the presence or absence of photosynthetic algae symbionts and the metabolic processes involved when forming new chambers during the individuals growth (Schiebel and Hemleben 2017; Takagi et al., 2020, 2021). The first process gives the species that host them a slight advantage as they modify the chemistry of the immediate water column, while the second mechanism is still under poorly constrained. However, I think that, as this is a review paper, these mechanisms should be described.

Response: Good point. We will refer the reader to Section 2.2.3 on Photosymbiosis where we discuss the impact of photosymbionts on calcification. We will note in the text *"It is clear that specific traits within ecogroups, for example symbiosis (see Section 2.2.3), can positively impact the calcification response due to modification of the calcifying microenvironment and provision of an energy subsidy (e.g., Köhler-Rink and Kühl, 2005)."* We will also describe briefly the second metabolic mechanism that is poorly known but largely relates to enhanced respiration near the site of calcification that ultimately increases the total pool of inorganic carbon in the cell for calcification (e.g., Bentov *et al*., 2009 and Scheibel and Hemleben, 2017). See Lines 236-244.

Lines 217-220. Too many concepts in one sentence makes it hard to read. Maybe separate?

Response: Thanks for flagging. We have split this sentence in two.

Lines 223-226. See previous comment, long and complex sentence. Maybe rephrase? Also, you consider this effects are "short-termed", however, the impact on the reproductive strategy, i.e. reduction of gamete/body size, is not that short term. Influencing the reproductive strategy of a species depends on a wide array of factors. I suggest rewriting this sentence in a more conservative approach.

Response: We have shortened and rephrased this sentence as suggested.

Lines 249-251. See previous comment surrounding spinose/non-spinose species feeding preferences. I think that the two paragraphs surrounding feeding strategy should be more homogenous.

Response: We have carefully reviewed the content of both paragraphs as suggested and updated the text to ensure better balance between the two feeding types.

Lines 261-262. This sentence lacks clarity, I would avoid mentioning "most zooplankton".

Response: We have removed reference to "*most zooplankton*".

Line 266. Considering the development of the paragraph, which is correct, I suggest to state that non-spinose species are omnivorous with a preference towards an herbivorous diet. By the way, that is what the reader could interpret by following the paragraph.

Response: We have updated the text as suggested.

Lines 284-286. Again, long and complicated sentence. Maybe separate in 2?

Response: We have simplified and rephrased the sentence to *"Model simulation of trophic dynamics requires data on prey preferences, e.g., prey acceptance rates, protein acquisition from zooplankton versus phytoplankton, and average digestion and capture times."*

Line 340. Certain morphotypes of G. bulloides have recently been described as hosting bacterial symbionts (i.e. Synecocchus) (Bird et al., 2017). I would also correct in Table 2 when G. bulloides is described as hosting "none" symbionts.

Response: Bird et al. (2017) describe the identified cyanobacteria (*Synecocchus*) as endobionts in *G. bulloides* as the metabolic (if any) role of these are not yet known, i.e., are they really symbionts? They have also currently only been reported in one genetic type of *G. bulloides* IId and only from the eastern Pacific Ocean so it is unclear how widespread any association might be. However, we it was an omission not to include any reference to this possibility in the main text and we have amended the main text in Section 2.2.3 on photosymbionts to include reference to the potential for a different type of symbiont, and particularly the need for future work to confirm the nature of the relationship (L407-408, L610). We have modified Table 2 to read "None*" under "Symbiont type" and describe the possibility of symbiosis in the caption.

Line 365. Whole paragraph. See previous comment regarding the effect of photosymbionts on calcification. Further elaborating about it, as suggested earlier, in this paragraph could also be a possibility.

Response: This is a good point and we will develop the text on how symbioses may support calcification as requested, e.g., L438-441, *"Algal photosymbionts also aid calcification by increasing the pH of the foraminifera's immediate microenvironment above ambient seawater by utilising CO_2 during photosynthesis and therefore potentially enhancing calcification."*

Technical comments

Response: We have made all of the suggested technical corrections in the final manuscript.

References cited in response

Ambrecht et al., Ancient DNA from marine sediments: Precautions and considerations for seafloor coring, sample handling and data generation. Earth-Science Reviews, 196, 102887, 2019.

Andres, K.J., Lodge, D.M. Andres, J. Environmental DNA reveals the genetic diversity and population studies of an invasive species in the Laurentian Great Lakes, PNAS, 120(37), e2307345120, 2023.

Barrenechea Angeles et al., Planktonic foraminifera eDNA signature deposited on the seafloor remains preserved after burial in marine sediments, Scientific reports, 10, 20351, 2020.

Bentov, S., Brownlee, C. and Erez, J. The role of seawater endocytosis in the biomineralization process in calcareous foraminifera, PNAS, 106(51), 21500-21504, 2009.

Bird, C. et al. Cyanobacterial endobionts within a major marine planktonic calcifier (*Globigerina bulloides*, Foraminifera) revealed by 16S rRNS metabarcoding, Biogeosciences, 14, 901-920, 10.5194/bg-14-901-2017, 2017.

Cao et al., Molecular diversity of foraminiferal eDNA in sediments and their correlations with environmental factors from the Yellow Sea, Estuarine, Coastal and Shelf Science, 313, 109076, 2022

de Vargas, C., et al. Ocean plankton. Eukaryotic plankton diversity in the sunlit ocean, Science, 348, 1261605, 10.1126/science.1261605, 2015.

Fraile, I. *et al.* Predicting the global distribution of planktonic foraminifera using a dynamic ecosystem model, *Biogeosciences*, 5, 891–911, https://doi.org/10.5194/bg-5-891-2008, 2008.

Fraile, I. *et al*. Modelling the seasonal distribution of planktonic foraminifera during the Last Glacial Maximum, *Paleogeography*, 24, PA2216, https://doi.org/10.1029/2008PA001686, 2009.

Gibbs, S. *et al*. Algal plankton turn to hunting to survive and recover from end-Cretaceous impact darkness. *Sci. Adv.* **6,** eabc9123.DOI:10.1126/sciadv.abc9123, 2020.

Grigoratou, M. et al. A trait-based modelling approach to planktonic foraminifera ecology, Biogeosciences, 16, 1469-1492, 10.5194/bg-16-1469-2019, 2019.

Hansen, B., Bjornsen P.K. & Hansen, P.J., The size ratio between planktonic predators and their prey, *Limnology and Oceanography*, 39, doi: 10.4319/lo.1994.39.2.0395, 1994.

Hoshino, T. and Inagaki, F. Distribution of eukaryotic environmental DNA in global subseafloor sediments, Progress in Earth and Planetary Science, 11(19), 2024.

Köhler-Rink, S. and Kühl, M. The chemical microenvironment of the symbiotic planktonic foraminifer *Orbulina universa*, Marine Biology Research, 1, 68-78, 10.1080/17451000510019015, 2005.

Lombard, F. *et al*. Modelling planktic foraminifer growth and distribution using an ecophysiological multi-species approach, *Biogeosciences*, 8, 853–873, https://doi.org/10.5194/bg-8-853-2011, 2011.

Malviya et al. Insights into global diatom distribution and diversity in the world's ocean, PNAS, 113(11), E15156-25, 2016.

Meilland, J. *et al*. Rare but persistent asexual reproduction explains the success of planktonic foraminifera in polar oceans, *Journal of Plankton Research*, 45, 15–32, https://doi.org/10.1093/plankt/fbac069, 2023.

Morey, A.E., Mix, A.C, & Pisias, N.G. Planktonic foraminiferal assemblages preserved in surface sediments correspond to multiple environmental variables. *Quaternary Science Reviews*, 24, 925-950, 2005.

Pawlowski, J., Lejzerowicz, F. and Esling, P., Next-Generation Environmental Diversity Surveys of Foraminifera: Preparing the Future, The Biological Bulletin, 227(2): 93-106, 2014.

Ruppert, K.M., Kline, R.J. and Rahman, M.S. Past, present, and future perspectives of environmental DNA (eDNA) metabarcoding: A systematic review in methods, monitoring, and applications of global eDNA. Global Ecology and Conservation., 17, e00547, 2019.

Rutherford, S., D'Hondt, S. & Prell, W. Environmental controls on the geographic distribution of zooplankton diversity. *Nature*, 400(6746), 749–753. https://doi.org/10.1038/23449, 1995.
Schiebel, R. and Hemleben, C. Planktic Foraminifers in the Modern Ocean, Springer, Heidelberg 2017.

Schmidt, D. N. *et al*. Size distribution of Holocene planktic foraminifer assemblages: Biogeography, ecology and adaptation. *Marine Micropaleontology*, 50(3–4), 319–338. https://doi.org/10.1016/S0377-8398(03)00098-7, 2004.

Schweizer, M. et al., Trophic strategies of intertidal foraminifera explored with single-cell microbiome metabarcoding and morphological methods: What is on the menu? Ecology and Evolution, 12, e9437, 10.1002/ece3.9437, 2022.

Ser-Giacomi, E. et al. Ubiquitous abundance distribution of non-dominant plankton across the global ocean, Nature Ecology and Evolution, 2(8), 1243-1249, 10.1038/s41559-018-0587-2, 2018.

Ying, R. et al. ForamEcoGEnIE 2.0: incorporating symbiosis and spine traits into a trait-based global planktic foraminiferal model, Geosci. Model Dev., 16, 813-832, 10.5194/gmd-16-813-2023, 2023.

Žarić, S., Schulz, M. & Mulitza, S. Global prediction of planktic foraminiferal fluxes from hydrographic and productivity data, *Biogeosciences*, 3, 187–207, https://doi.org/10.5194/bg-3-187-2006, 2006.